ANALYSIS

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ON THE LOGICAL POSITIVISTS' THEORY OF TRUTH[1]

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THIS paper has been suggested by a recent discussion between Prof. Schlick and Dr. Neurath, made public in two articles which appeared in volume 4 of *Erkenntnis*,[2] which mainly concerns the positivistic concept of verification and truth.

For the following exposition it may be advantageous to refer to the well known crude classification which divides the different theories of truth into two main groups, i.e. the correspondence-theories and the coherence-theories of truth. According to the correspondence-theories truth consists in a certain agreement or correspondence between a statement and the so-called " facts " or " reality " ; while according to the coherence-theories truth is a possible property of a whole system of statements, i.e. a certain conformity of statements with each other ; in extreme coherence-theories truth is even identified with the mutual compatibility of the elements of such a system.

The Logical Positivists' theory of truth developed step by step from a correspondence-theory into a restrained coherence-theory. Let us shortly consider the most important logical phases of this process (which do not exactly correspond to the historical ones).

The philosophical ideas which L. Wittgenstein has developed in his *Tractatus Logico-Philosophicus*, and which represent the logical and historical starting point of the Viennese Circle's

[1] It has unfortunately been necessary to condense Dr. Hempel's paper slightly. Editor.

[2] M. Schlick : *Ueber das Fundament der Erkenntnis, Erk.* 4, 79.
O. Neurath : *Radikaler Physikalismus und " wirkliche Welt," Erk.* 4, 346.

researches, are obviously characterised by a correspondence theory of truth.

According to one of Wittgenstein's fundamental theses a statement is to be called true, if the fact or state of affairs expressed by it exists ; otherwise the statement is to be called false. Now, in Wittgenstein's theory the facts constituting the world are conceived to consist ultimately of certain kinds of elementary facts which are not further reducible to other ones. They are called atomic facts, and those which are composed of them, molecular facts. In correspondence to these two kinds of facts, two kinds of statements are assumed : atomic statements to express the atomic facts, and molecular statements to express the molecular ones. The logical form in which a molecular statement is constituted by atomic ones reflects the formal structure of facts ; and, consequently, just as the existence or non-existence of a molecular fact is determined by the existence or non-existence of its atomic constituents, the truth or falsity of a molecular statement is determined by the corresponding properties of the atomic statements ; that is to say : each statement is conceived to be a truth-function of the atomic statements.

Wittgenstein's ideas concerning truth were rather generally adopted by the early Viennese Circle. The first to raise doubts, which soon developed into a very energetic opposition, was Dr. Neurath. The first who recognised the importance of Neurath's ideas was Prof. Carnap. He joined some of his most important theses and gave them a more precise form, and he and Neurath, mutually exciting each other, developed these ideas to the theory of truth which we shall deal with.

A crude, but typical formulation of Dr. Neurath's main theses may be given as follows.[3]

Science is a system of statements which are of one kind. Each statement may be combined or compared with each other statement, e.g. in order to draw conclusions from the combined statements, or to see if they are compatible with each other or not. But statements are never compared with a " reality," with "facts." None of those who support a cleavage between statements and reality is able to give a precise account of how a comparison

[3] See *Soziologie im Physikalismus, Erk.* 2,393 : (2) *Physikalismus, Scientia,* Nov. 1931 : (3) *Sozialbehaviorismus, Sociologus* 8,281 (1932) : (4) *Einheitswissenschaft und Psychologie,* in series *Einheitswissenschaft,* Gerold, Vienna 1933 : (5) *Protokollsätze, Erk.* 3,204.

between statements and facts may possibly be accomplished, and how we may possibly ascertain the structure of facts. Therefore, that cleavage is nothing but the result of a redoubling metaphysics, and all the problems connected with it, are mere pseudoproblems.

But how is truth to be characterized from such a standpoint? Obviously, Neurath's ideas imply a coherence theory.

Carnap developed, at first, a certain form of a suitable coherence theory, the basic idea of which may be elucidated by the following reflection: If it is possible to cut off the relation to "facts," from Wittgenstein's theory and to characterize a certain class of statements as true atomic statements, one might perhaps maintain Wittgenstein's important ideas concerning statements and their connexions without further depending upon the fatal confrontation of statements and facts, and upon all the embarrassing consequences connected with it.

The desired class of propositions presented itself in the class of those statements which express the result of a pure immediate experience without any theoretical addition. They were called protocol-statements, and were originally thought to need no further proof.

Replacing the concept of atomic statements by that of protocol statements was the *first* step in abandoning Wittgenstein's theory of truth.

A change of view concerning the formal structure of the system of scientific statements represents the *second* step of the evolution leading from Wittgenstein's theory of truth to that of Carnap and Neurath.

According to Wittgenstein, a proposition which cannot ultimately be verified, has no meaning; in other words, a statement has a meaning when and only when it is a truth-function of the atomic propositions.

The so called laws of nature, as will be illustrated below, cannot entirely be verified; therefore they represent according to the *Tractatus* no statements at all, but mere instructions for establishing meaningful statements.

But when developing the theory I am speaking of, Carnap took into account that in science empirical laws are formulated in the same language as other statements, and that they are combined with singular statements, in order to derive predictions.

Therefore he concluded that Wittgenstein's criterion for meaningful statements was too narrow, and must be replaced by a wider one. He characterises empirical laws as general implicative statements which differ by their form from the so-called singular statements, such as " Here is now a temperature of 20 degrees centigrade."

A general statement is tested by examining its singular consequences. But as each general statement determines an infinite class of singular consequences, it cannot be finally and entirely verified, but only more or less supported by them : a general statement is not a truth-function of singular statements, but it has in relation to them the character of an *hypothesis*. The same fact may be expressed as follows : a general law cannot be formally deduced from a finite set of singular statements. Each finite set of statements admits an infinite series of hypotheses each of which implies all the singular statements referred to. So, in establishing the system of science, there is a conventional moment ; we have to choose between a large quantity of hypotheses which are logically equally possible, and in general we choose one that is distinguished by formal simplicity as Poincaré and Duhem often accentuated.

Further it is important to recall, that the singular statements have themselves the character of hypotheses in relation to the protocol-statements, as Carnap shows in " Unity of Science." Let us note, that consequently even the singular statements which we adopt, which we regard as true, depend upon which of the formally possible systems we choose.

Our choice is logically arbitrary, but the large number of possibilities for choosing is practically restricted by psychological and sociological factors, as particularly Neurath emphasizes.

Thus also a second fundamental principle of the " Tractatus " must be abandoned : it is no longer possible to define truth or falseness of each statement in terms of truth or falseness of certain basic statements, they may be atomic statements or protocol statements or other kinds of singular statements. For even the usual singular statements were revealed to be hypotheses in relation to the basic statements. Now a hypothesis cannot fully and finally be verified by a finite series of singular statements; a hypothesis is not a truth-function of singular statements, and consequently a singular statement which is not a

basic statement itself is not a truth-function of basic statements.

So the refined analysis of the formal structure of the system of statements involves an essential loosening or softening of the concept of truth ; for according to the considerations just mentioned, we may say : In science a statement is adopted as true if it is sufficiently supported by protocol statements. So there occurs in science, one drops at least one of the mentioned protocol statements.

And that characterizes an essential trait which the theory considered here still has in common with Wittgenstein's view : the principle of reducing the test of each statement to a certain kind of comparison between the statement in question and a certain class of basic propositions which are conceived to be ultimate and not to admit of any doubt.

The *third* and *last* phase of the logical evolution here considered may be characterized as the process of eliminating even this characteristic from the theory of truth.

Indeed, as Dr. Neurath emphasized rather early, it is very well imaginable that the protocol of a certain observer contains two statements which contradict each other, say " I see this patch entirely dark-blue and also entirely light-red." And if that occurs in science, one drops at least one of the mentioned protocol statements.

So protocol statements can no more be conceived as constituting an unalterable basis of the whole system of scientific statements, though it is true that we often go back just to protocol statements when a proposition is to be tested. Or, as Neurath says : We don't renounce a judge who decides if a statement in question is to be adopted or to be rejected; this judge is represented by the system of protocol statements. But our judge is removable. Carnap supports the same view, saying : There are no absolutely first statements for establishing Science ; for each statement of empirical character, even for protocol statements, further justification may be demanded, e.g. the protocol statements of a certain observer may be justified by statements contained in the report of a psychologist who examines the reliability of the observer before or even while he makes his observations.

So there may be attached to any empirical statement a chain of testing steps in which there is no absolute last link. It depends

upon our decision when to break off the testing process, and it is no more exact to compare science with a pyramid rising on a solid basis. Neurath rather compares science with a ship which is perpetually being altered in the open sea, and which can never be put into a dry dock and reconstructed from the keel upwards.

Obviously, these general ideas imply a coherence theory of truth. But it must be emphasized that by speaking of statements only, Carnap and Neurath do by no means intend to say: "There are no facts, there are only propositions"; on the contrary, the occurrence of certain statements in the protocol of an observer or in a scientific book is regarded as an empirical fact, and the propositions occurring as empirical objects. What the authors do intend to say, may be expressed more precisely thanks to Carnap's distinction between the material and the formal mode of speech.[4]

As Carnap has shown, each non-metaphysical consideration of philosophy belongs to the domain of Logic of Science, unless it concerns an empirical question and is proper to empirical science. And it is possible to formulate each statement of Logic of Science as an assertion concerning certain properties and relations of scientific propositions only. So also the concept of truth may be characterized in this formal mode of speech, namely, in a crude formulation, as a sufficient agreement between the system of acknowledged protocol-statements and the logical consequences which may be deduced from the statement and other statements which are already adopted.

And it is not only possible but much more correct to employ this formal mode rather than the material one. For the latter involves many pseudoproblems which cannot be formulated in the correct formal mode.

Saying that empirical statements "express facts" and consequently that truth consists in a certain correspondence between statements and the "facts" expressed by them, is a typical form of the material mode of speech.

The pseudoproblems connected with it are still alive in many an objection raised against Carnap's and Neurath's ideas; this is true also for some objections expounded in Prof. Schlick's paper

[4] Carnap: *Logische Syntax der Sprache*, Vienna, 1934; and *Philosophy and Logical Syntax*, lectures given in London, 1934, reported *Analysis*, vol. 2, no. 3; *The Unity of Science*, *Psyche Miniatures* 63, London 1934.

(and for some considerations rather similar to them which B. v. Juhos recently developed).[5]

Prof. Schlick begins by raising the objection that radically abandoning the idea of a system of unalterable basic statements would finally deprive us of the idea of an absolute ground of knowledge and lead to a complete relativism concerning the problem of truth.

But we must reply that a syntactical theory of scientific verification cannot possibly give a theoretical account of something that does not exist in the system of scientific verification. And indeed, nowhere in science will one find a criterion of absolute unquestionable truth. In order to have a relatively high degree of certainty, one will go back to the protocol statements of reliable observers ; but even they may give place to other well supported statements and general laws. So demanding an absolute truth criterion for empirical statements is inadequate ; it starts from a false presupposition.

We may say that searching for a criterion of absolute truth represents one of the pseudo-problems due to the material mode of speech : indeed the phrase that testing a statement is comparing it with facts, will very easily evoke the imagination of one definite world with certain definite properties, and so one will easily be seduced to ask for the one system of statements which gives a complete and true description of this world, and which would have to be designated absolutely true. By employing the formal mode of speech, the misunderstanding which admits no correct formulation disappears, and with it the motive for searching for a criterion of absolute truth.

Prof. Schlick assumes an absolutely solid ground of knowledge ; but on the other hand he concedes that it is advantageous in a theory of truth to consider only propositions. There is therefore only one way left for him to characterise truth, that is : to assume that there is a certain class of statements which are synthetic, and nevertheless absolutely, unquestionably true, by comparison with which every other statement might be tested. And in fact Prof. Schlick assumes that there are statements of this character ; he calls them " Konstatierungen " (" statings "), and attributes to them the form " Here now so and so," e.g.

[5] B. v. Juhos : *Kritische Bemerkungen zur Wissenschaftstheorie des Physikalismus* Erk. 4, 397.

" Here now blue and yellow side by side " or " Here now pain."

But Prof. Schlick himself concedes that any scientific statement is an hypothesis, and may be abandoned, and therefore he is obliged to assume that his unabandonable " Konstatierungen " are not scientific statements, but that they represent the inducement for establishing protocol statements corresponding to them ; e.g. " The observer Miller saw at that time and that place blue and yellow side by side."

Concerning these " Konstatierungen," Prof. Schlick claims that (1) as distinct from ordinary empirical statements, they are understood and verified in *one* act, i.e. by comparing them with facts. So he returns to the material mode of speech, and he even describes " Konstatierungen " as the solid points of contact between knowledge and reality. The embarrassing consequences of such a form of consideration were pointed out just now. (2) Prof. Schlick assumes that " Konstatierungen " cannot be written down like ordinary statements, and that they are valid only in one moment, i.e. when they are established. But then it is impossible to understand how a " Konstatierung " may be compared with an ordinary scientific statement. And such a comparison would be necessary, as Prof. Schlick assumes that every empirical statement is in the end tested by " Konstatierungen."

But it is important still to discuss the starting consideration of Prof. Schlick's ideas. This is the following one :

Carnap's and Neurath's thesis, that in science a statement is adopted as true if it is sufficiently supported by protocol statements, leads into nonsense if the idea of absolutely true protocol statements is rejected ; for obviously one may fancy many *different* systems of protocol statements and of hypothetical statements sufficiently supported by them ; and according to Carnap's and Neurath's formal criterion, each of these different systems, which may even be incompatible with each other, would be true. For any fairy tale there may be constructed a system of protocol statements by which it would be sufficiently supported ; but we call the fairy tale false and the statements of empirical science true, though both comply with that formal criterion.

In short : What characteristics are there according to Carnap's and Neurath's views, by which to distinguish the true protocol

statements of our science from the false ones of a fairy tale?

As Carnap and Neurath emphasize, there is indeed no formal, no logical difference between the two compared systems, but an *empirical* one. The system of protocol statements which we call true, and to which we refer in every day life and science, may only be characterized by the historical fact, that it is the system which is actually adopted by mankind, and especially by the scientists of our culture circle; and the "true" statements in general may be characterized as those which are sufficiently supported by that system of actually adopted protocol statements.[6]

The adopted protocol statements are conceived as spoken or written physical objects, produced by the subjects just mentioned; and it might be possible that the protocol statements produced by different men would not admit the construction of one unique system of scientific statements, i.e. of a system sufficiently supported by the whole set of protocol statements of different people; but fortunately this possibility is not realized: in fact, by far the greater part of scientists will sooner or later come to an agreement, and so, as an empirical fact, a perpetually increasing and expanding system of coherent statements and theories results from their protocol statements.

In a reply to an objection of Zilsel,[7] Carnap[8] adds a remark which perhaps provides us a possibility of explaining that fortunate empirical fact.

How do we learn to produce "true" protocol statements? Obviously by being conditioned. Just as we accustom a child to spit out cherry-stones by giving it a good example or by grasping its mouth, we condition it also to produce, under certain circumstances, definite spoken or written utterances, e.g. to say "I am hungry" or "This is a red ball."

And we may say that young scientists are conditioned in the same way if they are taught in their university courses to produce under certain conditions, such utterances as "The pointer is now coinciding with scale-mark number 5" or "This word is Old-High-German" or "This historical document dates from the 17th Century."

[6] So truth is not reduced without qualification to formal properties of a system of statements: Carnap and Neurath do not support a pure coherence-theory, but, as we expressed it at the beginning, a restrained coherence-theory of truth.

[7] Zilsel: *Bemerkungen zur Wissenschaftslogik*, Erk. 3, 143.

[8] Carnap: *Erwiderung auf Zilsel und Duncker*, Erk. 3, 177.

Perhaps the fact of the general and rather congruous conditioning of scientists may explain to a certain degree the fact of a unique system of science.

The evolution of the concept of truth we considered is intimately allied to a change of view concerning the logical function of protocol statements. Let me finish with some remarks relating to this point.

Originally, Carnap introduced the concept of protocol statements to denote the basis of testing empirical statements; in divorce from Wittgenstein's principles, he showed that even singular statements have the character of a hypothesis in relation to the protocol statements: they cannot be finally verified but they may only be more or less confirmed by them. And there is no precise rule stipulating a minimum-degree of confirmation as necessary for a statement to be adopted: in the end, the adoption or the rejection of a statement depends upon a decision.

And in the recent form of Carnap's and Neurath's theory protocol statements are still more radically divested of their basic character: they lose the irremovability originally attributed to them; even the protocol statements are revealed to be hypotheses in relation to other statements of the whole system; and so a protocol statement, like every other statement, is at the end adopted or rejected by a decision.

So I think that there is no essential difference left between protocol statements and other statements.

Dr. Neurath proposes to confine the term "protocol-statements" to statements of a certain form, i.e. to those in which the name of an observer and the result of an observation occurs. Hereby he will accentuate the empirical character of science, in which a thorough testing is mostly traced back to observation statements.

Prof. Carnap on the other hand accentuates that (1) a test is not in all cases traced back to such observation-statements (2) that observation-statements of the kind Dr. Neurath means, may themselves be tested by a reduction to statements, even of a different form. And (3) he emphasizes that in any case determining the formal characteristics of protocol statements is a matter of convention, not a question of fact. He illustrates this point of view by sketching three different conventions, each of

which may be chosen in order to characterize formally a class of protocol statements. One of these conventions has been suggested by Dr. Popper ; it consists in admitting statements of any form to figure as protocol statements. Prof. Carnap finds Dr. Popper's convention to be the most suitable and most simple of the three conventions discussed by him. And indeed I think that just this convention suits in the most perfect manner Carnap's and Neurath's general views of verification and truth.

Thus the concept of protocol statements may have become superfluous at the end. But it was at least a most essential auxiliary concept, and relativising or fully abandoning it represents the last step in an extended theoretical development.

Finally we consider the consequences this evolution has for the problem of atomic facts, which plays a considerable part for Wittgenstein's theory.

Thanks to correctly expressing the problems to be solved in the formal mode of speech the double question of what are atomic facts, and what are atomic statements, is revealed to be only one question, at first expressed in the material and then in the formal mode.

So there remained only one problem, i.e. to find the structure of the atomic statements, or in the version due to Prof. Carnap : to find the logical form of protocol statements. This problem was at first, e.g. in " Unity of Science " conceived to be a question of fact ; but then, Carnap's considerations led to the result that the form of protocol statements cannot be found, but must be fixed by a convention. And this insight eliminates from the Logical Positivist's theory of verification and truth a remainder of absolutism which is due to metaphysical tendencies, and cannot be justified by a correct syntactical analysis of science.

Brussels, December 1934.

TWO KINDS OF ANALYSIS

By A. C. EWING

THIS article is not an attempt at an analysis of "analysis," nor is it intended as a criticism of the analytic school or any member of it. What I propose to do is merely to distinguish two different ways in which "analysis" has in fact been used by members of the school. In both cases what is analysed may be truly described as the meaning of certain words, but it seems to me that there is a relevant ambiguity in the phrase, *what a word* or *what a statement means*.

1. What is analysed may be what the persons who make a certain statement usually *intend to assert*. In that case the process of analysis will consist in finding a form of words which states better what the plain man really wanted to say but could not put so clearly. An indubitable example of analysis in this sense would be Mr. Russell's analysis of the proposition "The author of Waverley is Scotch." It just puts in a clearer form what we all undoubtedly would intend to say when we make this statement. (By "clearer" I mean "clearer for philosophical purposes." I do not intend to exclude the possibility that an expression which was clearer for philosophical purposes might be less clear for ordinary purposes.)

2. What is analysed may be not what we intended to assert in a statement but the qualities, relations and species of continuants mentioned in the statement. Both (1) and (2) may be covered by the phrase "analysis of what is meant by the statement," but I submit that they are different. I am afraid I can only make this point clear by the help of examples. Suppose for the sake of argument that the representative theory of perception is true and the correct analysis of "I see a tree" is said to be "I perceive a visual sense-datum standing in a certain relation r to a physical tree." (To make the analysis complete, "r" and "physical tree" would no doubt have to be defined further in a way which excluded among other things the view that the sense-datum was a spatial part of the surface of the actual tree). This is undoubtedly an example of analysis in a sense in which the term analysis is commonly used by members of the analytic school, whether

they take it to be a good or a bad analysis of " I see a tree." But it seems to me obvious that, whether it is a good or a bad analysis of the relation of visual perception, it cannot possibly express what the plain man who makes the statement intends to assert at all and is therefore quite different from an analysis in the first sense of the word. For a person who makes the statement " I see a tree " is not usually intending to assert anything about the representative theory of perception or the relation of his sense-datum to a tree. He takes for granted that we are familiar with the operation he describes as perceiving, but he does not intend to assert what relations are included in perceiving or whether perceiving does or does not involve a sense-datum in distinction from the tree. On the other hand, if we revert to examples of the first kind of analysis, when Mr. Russell says that " The author of Waverley is Scotch " is to be analysed as " There is one and only one person who wrote Waverley and this person is Scotch," or when Professor Moore says that " A is the brother of B " is to be analysed as " A is male, and A and B have common parents," they are certainly intending to express and in fact expressing what people who make these statements ordinarily intend to assert by them. When we say " The author of Waverley is Scotch " we all do intend to assert that there is only one person etc., if we are using words at all consistently, and when we say that A is the brother of B we all do intend to assert that A is male and that A and B have common parents. But when the plain man, or the philosopher in his unphilosophical moments, says " I see a tree," he is not intending to make any assertion as to whether perception is direct or representative. It follows therefore that the analysis given of " I see a tree " even if it is correct, and the first kind of analysis, are not related in the same way to what the plain man intends to assert. To take a still clearer instance, mentioned by Mr. Braithwaite as an example of analysis,[1] if a philosopher asserts that the tree is really a colony of souls he is giving an analysis of the nature of the tree for which there may be some philosophical arguments, tho' I personally do not think there are any valid ones, but he is not offering an analysis of what I intend to assert when I say " I see a tree." Even if Leibniz's philosophy were true exactly as it stands, it would not alter what the plain man intends to assert when he says

[1] *Cambridge University Studies*, pp. 3–4.

" I see a tree," and it is quite certain that he does not intend to assert that he is seeing, or is in any other relation to, a group of rudimentary souls. I do not believe that the tree is really a group of souls, but it certainly would be logically possible for me to hold that it was without in any way contradicting the fact that the plain man when he talks about trees does not intend to talk about groups of souls, a fact which Leibniz himself would have no doubt been perfectly ready to admit. The same would hold if I substituted for souls electrons or sense-data, or any account that you may prefer of what physical objects really are. For we must not suppose that, because qr is a correct analysis of p, therefore " S is qr " is necessarily a correct analysis of what we mean when we say that S is p, if " mean " signifies " intend to assert," as it undoubtedly does in one of its most important senses. For the proposition that S is p may be asserted by somebody who has not the slightest idea that p = qr or positively disbelieves this. I think the chief reason why this can be done is because he may have experienced the quality or relation p without knowing what its constituent elements are, and so he may make a true assertion ascribing p to something without intending to assert what the constituent elements of p are and while holding a totally wrong belief on this matter.

One important difference between the two kinds of analysis is that with the first kind it is a relevant objection to a particular analysis if on careful consideration we feel convinced that " we did not mean (intend to assert) this but something quite different," but with the second kind such an objection would be quite irrelevant. No one could reasonably reject the view that physical objects were colonies of souls simply on the ground that when we talk about physical objects in ordinary life we do not intend to assert that they are colonies of souls (though it would be an objection to the soul-theory if the proposition that they are colonies of souls did not merely go beyond common-sense propositions but actually contradicted the latter). It is an objection to an analysis that it does not give what we intended to assert only if it is meant as an analysis of what we intended to assert, not if it is meant as an analysis of the qualities, relations or species of continuants mentioned in the assertion.

Opponents of the analytic school often object that their analysis is concerned merely with the meaning of words, implying that it

is merely verbal in a sense which makes it relatively trivial. But, as we have seen, "the meaning of words" is ambiguous. It may signify what we intend to assert by the words, or it may signify the objective characteristics etc. meant, and in the latter sense not only analytic philosophy but any philosophy whatever must be concerned entirely with the meaning of words. The first kind of analysis, the analysis of what statements mean in the sense of what they are intended to assert, is indeed likewise neither valueless nor trivial, but I am inclined to think that it is valuable mainly not *per se* but as a means to an end. And this in two ways. In the first place the analysis of what is meant in this sense may be an essential preliminary to avoiding confusions which may otherwise form the ground of fallacious arguments, as has so often been the case in philosophy. But, secondly, a very special importance attaches to the first kind of analysis if we hold, as Professor Moore does, that the philosopher ought to accept "common-sense propositions" as true independently of any philosophical argument. For then it is very important to state, if possible, exactly what the propositions are which the philosopher should take as true on common-sense grounds, and this, which is by no means as obvious and easy a task as a tiro in philosophy might imagine, will be the work of the first type of analysis. For instance, if the philosopher must accept as true the common-sense proposition that there are chairs in this room, it is very important to determine what this proposition really is. The result of the first will limit the second kind of analysis, for the latter must not contradict the common-sense proposition the exact character of which the first type of analysis determines, but, while it must not contradict the first, the second may go beyond the first. Thus, if the first type of analysis led to the realist conclusion that by chairs and other physical things were meant things which possess certain spatial characteristics in the sense-given sense independently of being perceived, it would follow that any further philosophical analysis of the nature of physical things must be in conformity with this realist view, but it might still supplement it by further propositions which were not identical with or constituent parts of any known to common-sense. Thus a philosopher who maintained that chairs were unextended souls would in that case be contradicting common-sense propositions, but if he maintained that they consisted of

souls without denying that they were also extended he would not be doing so but would merely be giving additional information beyond any contained in common-sense propositions about physical things. He would indeed be contradicting a common-sense belief about philosophical subjects, because it is generally supposed by people who are not philosophers that inorganic objects do not consist of souls; but it is common-sense propositions about non-philosophical every-day matters, not common-sense beliefs about philosophical subjects, which have to be respected by the philosopher. On the other hand, if the first kind of analysis as applied to propositions about physical objects led to a phenomenalist conclusion, the realist could no longer claim that his opponents contradict common-sense propositions, and would therefore lose what would otherwise be the strongest argument in his favour. The first kind of analysis is therefore highly relevant to the second, but it is not identical with the second. Further, the second may give additional information about the objective world not contained in any common-sense propositions, e.g. that perception is representative or that a number is a class of classes ; the first cannot do so.

These are my present conclusions about the two kinds of analysis : to represent them accurately I should have been obliged indeed to insert a great number of expressions such as—"I am inclined to think" or "perhaps most probably"—signifying uncertainty, but this would have involved a tedious repetition, so I insert these expressions now once for all. Even if I have stated it dogmatically my contention is meant to be essentially tentative and not dogmatic in character.

Cambridge, October 1934